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## EXPEDIENCY VS. PRIORITY IN NOMENCLATURE

by

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The first chapter of the *International Code of Botanical Nomenclature* [1]) consists of "general considerations, guiding principles, and definitions." First among these we read that "botany cannot make satisfactory progress without a precise system of nomenclature which is used by the great majority of botanists in all countries."

This statement is patently untrue. It is easy to show (and it is shown below) that botanists have almost never agreed upon a precise system of nomenclature; certainly not before 1930. Between 1867 and 1930 there were times in which four systems of nomenclature competed for the acceptance of botanists [2]); and even the current rules, which the vast majority of botanists are "disposed to accept", have proved anything but precise. Yet those who would maintain that during this period botany did not make "satisfactory progress" must be hard to satisfy. One need only call to mind such names as De Bary, Boysen-Jensen, Burrill, Darwin, Goebel, von Sachs, Strasburger, de Vries, Warming; or, if "botany" here means only systematic botany, then Ascherson, Bentham, Britton, de Candolle, Engler, Gray, Hegi, J. D. Hooker, Komarov ... Was the work of these men unsatisfactory? The answer depends, of course, on one's expectations. But I doubt whether it would have been of much greater value even if a single precise code of nomenclatural rules had prevailed.

Rules of nomenclature began with Linnaeus; he established the sorts of names which we still use. But in deciding between competing names his rules relied chiefly on etymology, grammar and propriety. A concept of nomenclature which we may recognize as more akin to our own appeared in 1813 with the publication of Augustin Pyramus de Candolle's *Théorie élémentaire de la botanique*. Here the principle of priority is clearly enunciated: "que le premier qui découvre un être ou qui l'enregistre dans le catalogue de la nature, a le droit de lui donner un nom, et que ce nom doit être nécessairement admis, à moins qu'il n'appartienne déjà à un autre être ou qu'il ne pèche contre les règles essentielles de la nomenclature" (p. 228). The "essential rules" were such matters as the necessity of using Latin, of placing the generic name before the specific epithet, of grammatical agreement, and of some degree of harmony between names and "essential character"; in short, the Linnaean principles. But the principle of priority was the decisive factor in any conflict, "un terme fixe, positif, qui n'admet rien ni d'arbitraire, ni de partial" (p. 250).

De Candolle was one of the most influential botanists of his day; but, after all, his rules of nomenclature were but the pronoucements of one individual, and not all botanists were ready to follow them. Moreover, he could not foresee the difficulties of those who came after. His son Alphonse Louis Pierre Pyramus de Candolle, writing in 1867, expressed the general dismay at the profusion of names applied to plants. Botanists were confronted by diversity in points of view on the limits of genera and species, and were seeking for some way of fixing names in the midst of an unceasing variation of facts and ideas. Horticulturists could no longer find their way in the abundance of new names and accumulated synonyms,

[1]) Lanjouw et al. 1952.
[2]) cf. Weatherby 1949.

a chaos which they had themselves helped to create [3]). In this atmosphere of discontent it was suggested at the Congress held in London in 1866 that future Congresses do something about nomenclature; and the committee on organization of the Paris Congress of 1867 asked Alphonse de Candolle to prepare a draft of a code of laws of nomenclature to serve as a basis of discussion. The result was the well-known *Lois de la nomenclature botanique.* These established anew the principle of priority, specified the categories of taxonomic groups and the kinds of names to be applied to each, and included regulations for the retention of names when groups are remodeled and for the citation of authority after names.

But even these laws did not end all difficulties and uncertainties in naming plants. Everyone accepted priority as guiding principle; but how far back should one go in searching for the first name? Binomial names began, clearly enough, in 1753; but when did generic names begin? The years 1737, 1752, and others (including those of Vergil and Pliny) had their champions. And it was all very well to speak of priority of names; what was the name of a species? Did the "specific name" (what we now call the specific epithet, the second part of the binomial) itself have a separate priority, or did dates only apply to the entire binomial? Lively and at times acrid discussion was carried on in the botanical literature of the late nineteenth century. Such authorities as Jean Müller (1874), Alphonse de Candolle (1877), Edward Lee Greene (1887), Nathaniel Lord Britton (1888) maintained the rights of the earliest epithet, no matter what genus it was published under. Teodoro Caruel (1877), on the other hand, Henry Trimen (1878), and William Philip Hiern (1878), with George Bentham (1879), Asa Gray (1887), James Britten (1888), and lesser lights admitted only complete names — binomials — to considerations of priority; the correct epithet of a species was that which was first published under the genus to which it was finally assigned. The latter group of botanists continually protested against the creation of unnecessary synonyms by the making of new combinations; their opponents pointed out that synonymy was chiefly a product of taxonomy, not of nomenclature, and was even a sign of health in the science; and why (to quote Britten himself in 1892) should a change of name be more confusing for literary reasons than for taxonomic? One side argued for expediency, the use of the "accepted" names; the other denounced "convenience" as an entirely unjustifiable principle on which to found a scientific nomenclature. The "amenities of discussion" were often lost, especially in trans-Atlantic exchanges in which the "eccentricities of the neo-American school of nomenclature" were criticized even by those who supported some of the principal American tenets (see Britten 1890a, 1890b).

It was inevitable also that with the remodeling of genera conflict should arise over citation of authority. Was one to cite the original author of the binomial even in a new combination, thus "making him say what he did not say"; or the author of the combination, thus denying proper credit to the man who first distinguished the species? Was the authority a bibliographic convenience or an act of homage to a discoverer? Was it a part of the name or merely the history of the name? And so on and so on. [4]) Among the American "eccentricities" was the introduction of the parenthetic method of citation which we use today. [5])

Jackson wrote in 1881, "It is the period of transition that is trying; let the original and true name be set forth once for all, and difficulties will vanish, but the attempt to uphold certain names because we are used to them must result in ultimate failure . . ." But in 1888 Charles Cardale Babington was still saying that there must be exceptions to priority, and excoriating the "botanical rag-collectors" who raked up obscure publications in order to attach their names to new binomials. (How modern all this sounds!) Matters came to a head in 1891 with the publication of Carl Ernst Otto Kuntze's *Revisio generum plantarum.* He repeated with new vigor the old arguments for rule as against convenience ("dem Bequemlichkeitsprincip steht das Ordnungsprincip gegenüber"), saying that the latter would lead to the same state of affairs as that which prevailed before Linnaeus. The so-called

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[3]) *Lois,* p. 3, 4.

[4]) cf. J. D. Hooker 1872 (p. vii), de Candolle & Cogniaux 1876, Jackson 1881, Britten 1888.

[5]) The first use of this device that I can find is in the Britton, Sterns and Poggenberg *Preliminary catalogue* of 1888; but it is reported to have been proposed in a meeting of the B.A.A.S. as early as 1842.

Kew Rule ("das Bentham'sche Nomenclaturprincip")[6] must, like everything else, be abandoned in favor of priority. But besides this he demonstrated that application of the principle of priority — strict adherence to the *Lois* of 1867, he claimed — would result in some 30,000 changes in specific names and about 800 in generic names. He proposed also a number of modifications in the Candollean rules, intended to make them more precise and to provide for situations not therein foreseen.

In the words of Alphonse de Candolle (1892), many botanists were terrified ("épouvantés") by the magnitude of the proposed changes, by the consequences of the Law according to Kuntze. German botanists, led by Ascherson, Engler, Schumann and Urban, circulated their own set of proposals, including a list of 81 generic names to be retained in spite of priority-genera of "more than a merely scientific interest, or that are very large."[7] Instead of strict adherence to principle, nomenclature was to compromise with convenience; the 81 exceptions formed an entering wedge of nomenclature according to "expediency". (How shocked Engler and his colleagues would have been by the present list of 958 conserved generic names — with many more to come!) In 1897 the Berlin group published their own set of rules, supplementary to the Candollean, based on the idea that extreme reform was impracticable ("der Weg der extremen Reformer in der Nomenclaturfrage *nicht weiter gangbar ist*"); they included what we should now call a recommendation against homonyms, the abandonment of generic names not "in general use" 50 years after publication, the priority of epithets, and concerned themselves greatly with spelling and capitalization. Tautonyms and erroneous names could be corrected.[8] In short, "the impulse that led to the acknowledgment of the right of priority was only the vivid desire to create a stabile [sic] nomenclature. If we see that by the absolute and unlimited observance of the principle we probably gain the contrary of what we intended, we, who have ourselves made the rules of priority as a law, have the right to amend the latter."[9]

Briquet also, in 1894, produced a set of proposals for modification of the *Lois*, dealing with validity of publication, nomina nuda, starting-points, a homonym rule which he said was clearly stated in the original rules, and superfluous names ("noms mort-nés"). De Toni and Voglino in 1887 pointed out some of the pairs of homonyms which were causing "great confusion in scientific studies."

On the other side — both of the question and of the Atlantic Ocean — a group of American botanists had meanwhile issued their so-called "Rochester code".[10] This was not really a code of nomenclature, but a set of resolutions designed to amend existing rules. Priority was announced as the fundamental principle of nomenclature, admitting of no exceptions. In determining priority one must begin with 1753. The oldest epithet, under any genus, must be maintained. Later homonyms were invalid. Parenthetic authority-citations prescribed. The appearance of the Rochester resolutions evoked a number of energetic protests by other Americans (often quoted with approval by English writers; see Britten 1895): Liberty Hyde Bailey, William Gilson Farlow, John Macoun, Thomas Meehan, Benjamin Lincoln Robinson, and others — carrying the torch list by Asa Gray ("personally I detest the use of a parenthesis and the use of the oldest specific name"[11]). The dissension in Europe was faithfully reproduced in the United States.

The International Congress held at Genua in 1892, for which the Rochester resolutions were prepared, took no action on them, although they formed the subject of discussion.[12]

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[6]) The "Kew Rule" was that rule, already referred to, which admits to consideration only a complete binomial. *Petroselinum vulgare* Lag. would be maintained as the earliest name of the species as long as it is held to belong to this genus, in spite of the prior existence of *Apium crispum* Mill. for the same species.

[7]) Ascherson et al. 1892; Ascherson 1892.

[8]) Engler et al. 1897. Many of the more unworkable and uninformed provisions on orthography in the third edition of the *International Rules* (Briquet 1935) had their origin in this German code.

[9]) Ascherson et al. 1892.

[10]) Fairchild 1892.

[11]) Quoted by Britten in 1895; see also Gray 1887.

[12]) See Penzig 1892; Underwood 1892a, 1892b.

The Congress did consider and accept the first three Berlin proposals; postponing action, however, on the controversial fourth, that concerned with the conservation of certain generic names. No action was taken at the Paris Congress of 1900, except to make provision for the appointment of a committee to make proposals at the following Congress.[13]) The session on nomenclaure of 2 October 1900 was marked principally by a vehement defense of the 1867 rules by Louis Édouard Bureau; in his opinion they needed little or no modification.[14])

In 1904 a new and much larger committee of American botanists (including almost all of the committee of 1892) published an entirely new code of botanical nomenclature, for submission to the Congress to be held in Vienna in the following year. This was the so-called "Philadelphia code".[15]) Its proponents felt that no satisfactory result could be obtained by the amendment and modification of the old rules of 1867 "for the reasons that their arrangement is not philosophical in the light of modern experience and knowledge" and that they were vague and did not admit of clear construction. They therefore proposed the abandonment of the old laws and the substitution of the new code. This was based on the Rochester resolutions; but its most important feature was the introduction of the concept of "nomenclatorial types".[16])

It was unfortunate that in some respects the code represented the views of a minority rather than the practice of the majority of botanists in the United States. For instance, it made mandatory the use of "subspecies" instead of "variety"; the principle of "priority of position" was included. Such provisions, though of minor importance, made the code as a whole unacceptable to most Europeans (as well as many Americans) and denied the type concept a fair hearing by itself. The Vienna Congress re-enacted the rules of 1867, with important amendments.[17]) Nomina generica conservanda were established, and a requirement was inserted that descriptions of new groups must be in Latin; both provisions were instantly resented in America. However, parenthetic citation of authority was recognized, the invalidity of later homonyms was established, the "Kew Rule" was finally disqualified. One cannot but be impressed by the number of "American eccentricities" that were adopted and the failure of the most distinguished names in systematic botany to be on the winning side. But the time was not ripe, apparently, for important innovations (acceptance of the type method was postponed until 1930). The spirit of compromise ruled; the desire to retain certain well-known names was already too strong in many botanists; and sentimental attachment to the phrases of the father of botanical nomenclature probably played a part. So the simplicity of strict priority was once and for all rejected.

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The preceding brief sketch of the history of botanical nomenclature was written not merely to demonstrate the absurdity of a statement in the *International Code*, but to illustrate a trend. During the last century botanists have worked under the influence of two competing principles: priority and conservation. According to strict priority and the American code,

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[13]) Perrot 1900.

[14]) The Rochester resolutions were slightly modified by amendment at Madison, Wisconsin, in 1893. It is interesting to notice the differences in the two published reports of this meeting. In the *Botanical Gazette* (Coulter et al. 1893) *four* amendments are listed; the meeting is reported under the heading "Proceedings of he Madison Botanical Congress"; in the *Bulletin of the Torrey Botanical Club* (Britton 1893) only *three* amendments appear, under "Proceedings of the Botanical Club, A.A.A.S.". The two reports are quite different; each contains material lacking in the other, though both were obviously reports of the same meeting.

[15]) See Arthur et al. 1904.

[16]) The word "type" had been used in the Rochester resolutions, but without its modern significance; as it had, indeed, by Rafinesque many years earlier, by Britten in 1886, and by Jackson in 1887. De Candolle in 1867 clearly foresaw the need for the type method (p. 52, 53), but without providing the solution.

[17]) See Briquet 1906. The American code was published in 1907 (Arthur et al.).

the earliest published name for a given taxon is (in principle) the right name; if through ignorance we do not use the earliest but a subsequently published name, the earliest is still the right name, and must, if and when it is discovered, displace all others. Conservation (or, before it attained legality, "expediency") maintains a name which is considered desirable regardless of the date of its publication or the existence of other names. During the latter half of the nineteenth century there was a growing insistence on the strict application of law in the naming of plants, a growing feeling that anything else was unworthy of the dignity of a science. This movement culminated in Kuntze, who not only demanded that nomenclature adhere to a principle, but demonstrated what such adherence meant. Many welcomed this clarification, and were ready to inaugurate the revolution. Others, however, sought to compromise with existing practice by incorporating in their rules numerous exceptions to them. Both sides claimed that only thus could stability be attained; neither side demonstrated any appreciable gain in stability. Perhaps Jackson and Britton were right in saying that after a brief period of confusion there would be no more trouble with names. Perhaps, however, in view of the continued activity even in these latter days of those who dig in obscure places for neglected names, the name-changing would have endured indefinitely — even apart from the manipulations of the systematist.

It is now too late for revolution. It would be useless to suggest the formulation and adoption of a new code based simply on priority. Botanists patched up the old rules in 1905 just in order to avoid such a revolution (which at that time would have been comparatively bloodless). The American code was jettisoned largely to preserve certain familiar names against the rules. The rules were again patched up, to some purpose, in 1930; the type method was tacked on, in spite of the antithesis between it and many of the older articles. We patched once more in 1950, still trying to eliminate the inherent contradictions between the phrases of 1867 and the science of the twentieth century.[18]) The lists of conserved generic names are still growing, and names of higher rank may now be conserved; the pressure to admit specific names to the same treatment is unrelenting.

What is the conclusion? Is it not evident that naming by rule — i.e. by priority and the rules necessary to administer priority — is constantly losing ground to the conservation of preferred names? And is it not also clear that the adoption of the type method gives conservation a wholly new meaning? A name is no longer defined, as it was for Alphonse de Candolle, by a description of a taxonomic group, with all the variation therein contained — i.e. by circumscription of a population. For the purposes of nomenclature a name is attached to a single specimen, or to a subordinate name which is ultimately connected with a single specimen. The name *Hydrocotyle ambigua* is associated with a certain plant collected by John Bradbury on the Mississippi River in 1810 and now preserved in the Academy of Natural Sciences of Philadelphia. No rule of priority can disturb that association, and no taxonomic manipulation can separate the epithet *ambigua* from that specimen; no conservation is necessary, other than what is already implied in the *International Code*, to maintain that name (or epithet) against all others for that specimen.

Competition between names arises in three ways: (1) a taxonomist brings two or more existing types into the same taxon; (2) he recognizes (or thinks he recognizes) that two or more descriptions of taxa, associated with names but not with types, apply to the taxon which he is studying; (3) both of the foregoing contingencies arise at once, i.e. types and untypified descriptions must compete among themselves and with each other.

Is it always best to select the prior name, with or without a type, and to designate a lectotype or neotype if no holotype is available? History has answered in the negative, at

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[18]) But, after all these revisions, much of de Candolle's wording is faithfully preserved. His first Article read: "L'histoire naturelle ne peut faire de progrès sans un système régulier de nomenclature, qui soit reconnu et employé par l'immense majorité des naturalistes de tous les pays." Those who followed him differed only in limiting their remarks to botanists. Another Article of the present Code which provokes thought is the fourth: "The essential points in nomenclature are: (1) to aim at fixity of names; . . . ." This goes back not to Alphonse de Candolle but to his father, who wrote in 1813, "Pour qu'une nomenclature puisse devenir universelle, il faut qu'elle soit fixe. . . ." (*Théorie élémentaire*, p. 228). In what sense can the names of taxa be "fixed"?

least for genera. We prefer to maintain a name in general use against an unknown prior name. How should we choose between names equally meritorious in this respect? Obviously it would be advantageous to choose one with a good type over one without a type or with a fragmentary or unidentifiable type. But this argument applies to species with even greater force. When *Vernonia illinoensis* Gleason and *Vernonia missurica* Rafinesque are united (as Palmer and Steyermark thought they should be[19]), what do we gain by adopting the prior name? Gleason's type is preserved in the Britton Herbarium of the New York Botanical Garden; a Rafinesquian fragment labeled *Vernonia missurica* may possibly exist in Paris or elsewhere, but more probably does not; if and when such a specimen does come to light, it may be that nomenclatural disposition must again be revised, to the detriment of stability. In short, if we could "conserve" the name *Vernonia illinoensis* over *Vernonia missurica* for as long as these should be applied to the same taxon, a well-typified name would be secure against further nomenclatural disturbance — for instance, against the discovery of yet another Rafinesquian name possibly applied to the same taxon.

Such "conservation" mounts in reality to no more than a system of registration of names and types, with decisions on the relative status of competing names. Moreover, there is no reason to limit such a register to names adopted in spite of priority. *All names could be registered.*[20]) (To create such a register would certainly require considerable time and clerical work but would not be otherwise impracticable, especially since we now have a more active international organization than ever before.) Nuttall's choice of the epithet *bulbosa* from the three epithets available to him for one species (*Hydrocotyle ambigua* Pursh, *H. composita* Pursh and *Sison bulbosum* Michx.) was presumably based on priority, but perhaps merely on what he considered the most appropriate; it does not matter; registration of the three names, their types, and preference for one of them based primarily on typification would stabilize the name as much as any name can be — i.e. until some taxonomist decides to merge *Erigenia* in some other genus or *Erigenia bulbosa* in some other species.

Under present rules, if I find in a little-used but early publication a forgotten epithet bestowed upon a scantily described plant by a traveler who made no collections, I can — indeed I must — adopt this epithet in place of the familiar but later one in current use, perhaps transferring it to a different genus and seeing my reward in my name as authority for the new combination. If, however, I am instructed to register the name, making known the lack of a type and so disqualifying it for competition with the familiar name, I am less likely to join the ranks of nomenclatural trouble-makers.

To sum up, a trend has been demonstrated away from priority as a decisive factor in the selection of names and towards conservation of names preferred on the basis of usage. It is here proposed to widen this trend on the foundation of the type method; to include satisfactory typification as a basis for choosing among competing names; and to extend this method to names in all categories.

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[19]) 1935, p. 654.

[20]) The current list of nomina generica conservanda contains many names which need no conservation under the present rules. They are included in the list to avoid future confusion.

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# ON NOMENCLATURE: THE "TYPE-METHOD"

by

León Croizat (Mérida)

*(continued from p. 107)*

The very least we may demand of the sponsors of a "Type Method" to be worth its salt is that they have clear in mind the concept of type, and prove capable to discriminate among its various aspects. In so demanding, we do not press philosophical or legal requirements beyond the rational capacity of the average botanist, rather, we beg that everybody at least knows what he is talking about, for in the contrary case it is useless to discuss. In sum, before patiently tolerating the flag-weaving of those who believe that the American Code clarified the concept of type, we should like to ask that the gentlemen sit down and ponder, for the contrary is certainly the truth.

Two concepts of type are known to every practising botanist. When one of their number asks a colleague to loan him the *type* of a certain species known to be extant in the latter's herbarium, the borrower evidently expects to receive a *specimen,* pressed or preserved in usual fashion. By the same token, a botanist may ask a more fortunate friend to send him copy of the original description or figure of a species, in the understanding that this description or figure substitutes for the specimen that may not be extant for any one of a great many reasons. Accordingly, the term *type* answers in the first place the idea of a tangible essential to determine the application of a name, whether specimen, description or figure.

A second, well known meaning of *type* is biological. No botanist will fail to understand an author writing, e.g., "It is unfortunate that the biological average of *Planta vulgaris* is poorly represented by the specimen in the original publication." Indeed, it may happen that the type, description or figure, rests upon an indifferent or somewhat aberrant specimen, and that, for instance, a group of forms properly to be understood as a single species and heavily pubescent are on the contrary represented in herbarium, or in the library, by an original with very little indument. It is manifest that the type-specimen and the biological type of the same aggregate may thus be at odds, which, let us notice, speaks well for the Briquetian understanding, that only a competent monographer is qualified to work out — so far as still possible — a compromis between the two. The original describer of a species may indeed know far less about it than his successors, and may thus not be best qualified to decide what is to be its type whenever he has open choice among several specimens or collections.

Finally, a third concept of type exists which, though easily recognizable and oftentimes currently recognized, has given rise nonetheless to fundamental errors. To illustrate: *Euphorbia heterophylla* L. is understood by Boissier (in DC. Prodr. 15(2): 72. 1862) to consist of four subspecific groups of unspecified rank, namely, *genuina, cyatophora, graminifolia* and *minor,* identified in succession by Greek letters from *alpha* to *delta.* The first of these groups, *genuina,* is the type-subspecies by current understanding, and, as such, is necessarily typified by one of the original Linnaean references i.e., Plukenet's or Morison's, possibly also Dissertatio Euphorbiarum's. A critical study of the Boissierian concept demands in addition a consideration of the specimens and references Boissier introduces, such as *Lindheimer 696, 697; Moritz 801,* etc. It is thus crystal-clear that the

designation of a type-subspecies does not call *as a first step* for the recognition of a certain specimen in preference to another; it calls, on the contrary, for the preliminary recognition of a certain taxonomic unit of subordinate rank (in this case, the trinomial *genuina*), which unit is only in a second time interpreted in the light of a suitable specimen, figure or description. When I say that *E. heterophylla* is typified by its subdivision *genuina* Boiss. 1862, I affirm nothing which a normally well informed botanist may fail to understand. It is taken for granted that a species is typified by that subdivision which precisely answers the original concept, whether already published (as *E. heterophylla genuina* Boiss., 1862) or still unpublished (as could be *Planta vulgaris vulgaris* unpublished in 1952, but legitimately to be published by John Doe in 2002). Under this subdivision may lie any number of specimens, descriptions or illustrations, which is finally to say that the designation of this particular subdivision is not tantamount to identifying as typical some one specimen, description or illustration in particular. The difference here may seem to the uncritical hardly worth mentioning, but it involves in reality *two very different concepts of type*, one physical (specimen, description, figure), the other nomenclatorial (subdivision identified, or to be identified, by a name). Problems of the knottiest may arise in actual practice which call for a sharp discrimination of these concepts, and abusing a nomenclatorial type may bring about results quite as unwelcome as mishandling a physical one, which we are soon to learn through factual examples.

In conclusion: Three are the concepts of type, which I may briefly refer to as physical, biological and nomenclatorial. Be it observed that all these concepts are eventually understood in the light of something tangible — specimen, description, figure — so that a whole family, quite as much as a single form, are in the end typified by this something tangible. No one, nevertheless, would think of confusing a family with a form on the ground that both, after all, hark back to a specimen. The specimen, or its equivalents, are *one concept;* the endless chain of taxonomic groups based upon this specimen, or its physical equivalents, are *quite another concept.*

With this clear before us, let us turn now to the pages of the American Code. Perusing its *"Section IV. Application of Names"* we read, "Canon 15. The nomenclatorial type of a genus or subgenus is the species originally named or designated by the author of the name ... The type is to be selected from a subgenus, section or other list of species originally designated as typical .... *Psilogramme* is typified by .... the second section *Eupsilogramme* .... not from .... the first section *Jamesonia*"; and further on, "Canon 14. The nomenclatorial type of a species or subspecies is the specimen to which the describer originally applied the name in publication ...."

Canon 15 — roundly violated as we are soon to learn by one of its original proponents — contains nothing which may be said to be new or striking, even less unknown to the generality of European taxonomists long before 1905. Canon 14 on its part embodies a gross confusion in concepts, mistaking a physical type (specimen, description and/or figure) with a nomenclatorial one (typical subdivision). Had the authors of Canon 14 thought themselves out in a critical manner about types they could not fail to see as *Eupsilogramme* is the nomenclatorial type of *Psilogramme* so must *Planta vulgaris* (var.) *vulgaris* be the nomenclatorial type of *P. vulgaris*. In the end, both *Psilogramme* and *P. vulgaris* come to rest upon physical types, so that — adopting the confusion in concepts advanced by the founding fathers of the American Code — we should consistently but falsely maintain that *Psilogramme,* too, has for its nomenclatorial type a specimen. The immediate result of the American blunder was that the new fangled transatlantic "Type Method" broke down next immediately to international presentation. Briquet finally reported upon it in these terms (Texte Synopt. Docum. Congrès Vienne 90. 1905), "The American authors d'une façon générale, entendent comme "type de nomenclature" la subdivision qui lorsqu'un groupe est divisé ou remanié doit conserver le nom primitif". This is not so at all; Britton and his friends understood as "Type de Nomenclature" not only the *pars typica* of a name (the like they did in Canon 15) but also a physical type (specimen, description, figure, spoken of in Canon 14), thoroughly confusing the two concepts. Briquet, misunderstanding them, gave them nevertheless credit for consistency, which they did not deserve in the first place. He did not take them for the poor amateurs they actually were in matters nomenclatural, and this was unfortunate. Had the American founding fathers of the Code been required

to think a little harder before going to record with halfbaked generalities a great deal of recrimination, boasting, quibbling, claiming and counterclaiming would have been nipped in the bud.

That Britton, to mention by name one of the most outspoken advocates of the American Code, was only a putterer in the field of nomenclature is thoroughly well known to critical students of his work. To his stubborn rejection of the Vienna International Rules the nomenclature and classification of the Cactaceae, for example, owes some of the knottiest problems in typification, not only, but the wilful, needless stultification of a long tradition. To mention but one case: Eleven valid and legitimate subgenera of *Echinocactus* were published by Schumann (Gesamtbeschr. Kakt. 291-292. 1898) including *Euechinocactus,* which has exactly the same status as *Eupsilogramme.* Bent upon dismembering *Echinocactus,* Britton and his associate Rose should in accordance with their own pet American Code (see Canon 15) have retained at least one of the original six species of *Euechinocactus* under a recast *Echinocactus.* I am not aware that they did this, for, to the best of my knowledge, they distributed them all (Cact. 3. 1922; 4. 1923) between *Ferocactus* and *Thelocactus.* Characteristically, it seems that there is no mention whatever of *Euechinocactus* even in the indices of the monumental Brittonian cactology, which may be due quite as much to oversight as to something else.

Few are the well-practised botanists who have not had the opportunity of learning on the other hand that the *Species lectotypicae propositae* in the third edition of the Rules of International Nomenclature cannot be taken seriously. No one acting with taste and discrimination in his own speciality (witness MacVaugh repeating in Bull. Torrey Cl. 71: 457. 1944, the *species lectotypica proposita* of *Jatropha*) can accept the dictation of somebody who, relying upon hard and fast *fiats* in lieu of a deep knowledge of the subject, takes on himself to settle what is to be the type, physical or nomenclatorial alike, of this or that ticklish group, whether it be a genus or a species.

The age-old European feeling that correct typification is not a fit subject of straightjackets, rather a discriminate handling of oftentimes complex biological and systematic issues on the part of a competent monographer, is doubtless vindicated by such examples as I have introduced in the understanding that examples of the same sort could easily be multiplied if space only allowed. In sum, let it be affirmed that, (1) Mismanaging the concept and application of nomenclatorial types (e.g., *Euechinocactus, Cactus mammillaris,* etc.) can bring about results quite as unwelcome — and perhaps more so on the whole — as the uncertainty in the application of some physical type; (2) The American Code never was a reliable guide to the proper understanding of a correct "Type Method", its own makers blundering indeed egregiously in the easiest of cases, there, in sum, where no European master would have blundered long before the days of Britton & Co.

Let us next turn to Art. 18 in the third edition of the Rules, which American botanists very proudly, but quite as mistakenly, extol as their gem in the otherwise supposedly rickety crown of ageing European nomenclature. This Article recites as follows: "The application of names of taxonomic groups is determined by means of *nomenclatural types.* A nomenclatural type is that constituent element of a group to which the name of the group is permanently attached as an accepted name or as a synonym. The name of a group must be changed if the type of the name is excluded (see Art. 66). The type of the name of an order or suborder is a family, that of the name of a family, subfamily, tribe or subtribe is a genus, that of a generic name is a species, that of the name of a species or group of lower rank usually a specimen or preparation. In some species, however, the type is a description or figure given by a previous author. Where permanent preservation of a specimen or preparation is impossible, the application of the name of a species is determined by means of the original description or figure. Note: The nomenclatorial type is not necessarily the most typical or representative element of the group; it is merely that element with which the name of a group is permanently associated. Examples: the type of the name *Malvales* is the family *Malvaceae;* the type of the name *Malvaceae* is the genus *Malva;* the type of the name *Malva* is the species *Malva sylvestris* L.; the type of the name *Polyporus amboinensis* Fries is the figure and description in Rumph. Herb. Amboin. VI, p. 129, t. 57, fig. 1."

This Article is thoroughly ludicrous, which I say advisedly and maintain against all

comers. In the first place, the author, or authors, of this epic *pasticcio* are quite as inept in their conceptual handling of types as ever were the founding fathers of the American Code. They speak, of course, of a nomenclatorial, or nomenclatural, type (e.g., family typifying an order; species typifying a genus, etc.); of a physical type (specimen, preparation, description or figure); of a biological type ("The most typical or representative element of the group", as spoken of in the Note) but hopelessly confuse the first two, precisely as did the American Code, failing in addition to properly qualify the third with the necessary clause, "The most typical or representative element of the group *in the biological sense*". In sum, this "Type-Method" right there fails, where the ultimate concept of what is, or may be, a type is up for critical, precise definition. The wording itself of this all is incompetent and dubious; if it be true that "A nomenclatural type is that constituent element of a group to which the name of the group is permanently attached", and that, accordingly, *Malva* and the Malvaceae cannot be set asunder ever, it remains to be seen why Art. 18 should specifically refer to Art. 66, which states "If the genus *Portulaca* were excluded from the family now known as *Portulacaceae,* the residual group could no longer bear the name *Portulacaceae* and would have to be renamed". Well, then: How may we suppose that *Portulaca* might be taken out of the Portulacaceae, when *Malva* can never be segregated out of the Malvaceae by explicit definition in the very Art. 18? These Articles, 18 and 66, compound nonsense on the face of their own wordings, and only then could they be defended, if we agree that, after all, the intelligent reader is to draw his own conclusion, which is not the earmark of a competent body of rulings, whatever its purpose and nature might be.

Further: It remains to be explained why, having elected the group centering around *Malva* as their example, from the order down to the species, the author, or authors, of Art. 18 should jump out of *Malva sylvestris* into *Polyporus amboinensis,* without so much as heeding the thoroughly normal contingency that a critically minded inquirer asks, what is the status of *Malva sylvestris* var. *sylvestris* (or its equivalents in nomenclature and typification). Is this trinomial to be deemed a "specimen", "preparation", "description" and/or "figure" or, peradventure, rather more a genuine "nomenclatorial type"? It seems in addition likely that the introduction of *Polyporus amboinensis* to the record in Art. 18 is anything but happy. Rumphius' figure has only then status in modern nomenclature insofar as Fries gave it currency, which is to say that Rumphius' figure has no special status other than that of the countless pre-Linnaean illustrations "legitimized", e.g., in the pages of the *Species Plantarum,* 1753. It thus remains to be seen what Art. 18 actually means when specifying that, "In some species, however, the type is a description or a figure given by a previous author". What *previous author,* and why?

Jammed into the modern Rules by incompetent sponsors, Art. 18 flatly conflicts — as we may rightfully anticipate — with other texts in the same Rules wherein type-subdivisions of species are mentioned which are not at all "specimens", "preparations", "descriptions" and/or "figures", witness Art. 30 ("Subdivisions of the same species .... based on the same type"). Rec. XVIII (subdivisions of a species which include the type either of a higher subdivisional name or of the specific epithet"); Rec. XXXV ("Subdivision which included the type of the specific epithet"). Throughout these provisos, whether mandatory or advisory, speaks the old lore, a lore thoroughly conversant with the concept of *genuine* nomenclatorial types in a subspecific rank, the old lore of the masters of Geneva and Europe which the halfbaked notions of Britton & Co. pretented to rule out as unbecoming a new "biological age". Much could be added, but to add would only spoil when tittering is in order. Alas, what a grandiose mess.

Compressed to its ultimate essences, Art. 18 could be made to read approximately thus, "Every taxonomic group is, or can be, typified by a group of immediately lower rank (nomenclatorial type); in the end, the sum total of these groups is, singly and/or jointly, restricted in application by reference to a specimen, preparation, description and/or figure (physical type), in the understanding that neither the nomenclatorial nor the physical type may be rue to the biological average, (or biological type) of the groups in question. Typifying and typified groups cannot be separated; e.g., *Malva* (typifying group) is forever integral part of the *Malvaceae* (typified group), quite as much as is to be integral part of the concept under a name the physical type originally so designated by the author of the name".

It can safely be advanced that such an Article as this would embody nothing which is more genuinely "American" than "European" because it contains only truisms of the most palpable sort, something, in sum, which has permeated practising botany in some form or other everywhere from the very start of binomial nomenclature. In this light, it could be objected that the inclusion of a remodelled Art. 18 in the Rules would mean very little in the end, which is precisely what Briquet and his well informed coworkers knew all along, and I myself hold to be hardly open to question. It could further be objected that, being as such meaningless, a better arranged Art. 18 should be implemented with a host of hard and fast dicta in the spirit of the old American Code in order to become meaningful in the choice of "Types", and the use of "cotypes", "syntypes", "lectotypes", "kleptotypes", and all manners of more or less useful and melanchonious "types". To this last objection I would for one take exception, for science in general, and nomenclature in particular, is not to be a dance of dervishes yelling themselves hoarse in quest of the perfect taxonomic paradise. Let us ponder the lesson afforded by the *Species lectotypicae propositae,* and take of botany rather more a critical than a mechanical view. The most minute Rules and Recommendations will never cover all cases, and it is fitting that a *body of laws,* whether botanical, zoological or otherwise, be approached with due reverence and understanding by gentlemen who are neither incompetent nor ignorant. The trouble with us today is not about this or that "Type Method", or the like, rather more with the circumstance that every Tom, Dick and Harry thinks he has a vested right of tinkering with profound principles and most critical issues in a thoroughly irresponsible spirit, preaching at the best like Britton and acting at the worst like Britton. In he end, if writing into the Rules a "Type Method" is to save us from the press of the incompetent and the ignorant, I am doubtless for it as the lesser evil.

NOTE. — Following my departure from the United States to Venezuela in the early months of 1947, I lost touch, almost as a matter of course, with the steady flow of news and events in the world of botany. It is only in these days (June 1953) that I was offered the opportunity of laying hands upon a copy of the International Code of Botanical Nomenclature adopted by the Congress of Stockholm in July, 1950. Whatever I had previously heard of the decisions of that Congress had been through occasional correspondence and in a fragmentary form.

The present contribution — to the exception of its Note (June 1953) — was written prior to my departure from the United States, that is, years before the Congress at Stockholm came to be held. It stands to reason that my criticisms were directed at the time against the Rules in vigor (Amsterdam Code, 1935), and because of this it might be thought that they now do not apply to the "Type Method" devised at Stockholm in 1950. The normal surmise is that mankind tends to progress, and that the rules of nomenclature of 1950 are better than those of 1935. This is certainly not the case, however, for confusion on the contrary spreads, and babel yawns in botany as everywhere else ever wider. The "Type Method" of 1950 (Arts. 18-21, with attending Notes and Recommendations) duplicates the confusion among the concepts of "type" which originated with the American Code, and vitiated the International Codes after 1930. The same rotten planks of former "Type Methods" turn up in the Stockholm Code, 1950, if disguised under a coat of definitions of *holotype, neotype, lectotype, syntype, paratype,* etc., and plastered over with rulings hopefully to insure that from now on everybody shall eat with a fork instead of with his fingers.

Were I interested in anything but fundamentals, I should give time over to the task of showing how inept is on occasion the new "Type Method" even as to its own pet definitions. For example: Art. 21 states that, "If it is later proved that.... a type, herbarium sheet or preparation, contains parts belonging to more than one taxon, the name must remain attached to that part (*lectotype*) which corresponds more nearly with the original description". Precepts of the sort are, of course, useless padding, quite as would be the "order" that a fork is properly to be held elsewhere than by the prongs: I would like to be shown a taxonomist who facing a "mixed" specimen accepts as "typical" that part of it going against the description. Moreover, it seems evident that if herbarium-material contains heterogeneous specimens, the one to which the original description applies is the *holotype* — not the

*lectotype*, the like Art. 21 claims — the balance of the material being to be rejected as an erroneous addition. The very concept of *lectotype* implies choice out of basically homogeneous material, which the authors of Art. 21, as quoted, hardly seem to know.

However, all this is peanuts, as we might say, and nothing else can be expected of "legislators" who are not clear as to fundamentals; to their efforts the dictum applies: The finer they split, the thicker they litter.

Beyond comparison more serious is the fact that Art. 21 still trumpets half a century later the hoary misstatement of the American Code that the "Nomenclatural type" of a species, or of a group below the species, is a "Single specimen". Pathetic is the circumstance that fifty years of "codification", international gatherings, etc., have not made a dent in the confusion originally perpetrated by Britton & Co. in the very concept of "type".

Considering that the mess still concocted in 1950 around this concept stems at bottom from the fundamental inability of certain presumptuous botanists *to think this concept out once for ever*, it is not surprising that articles in the Stockholm Code other than those of the "Type Method" so called suffer disfigurement because of this same inability. To illustrate: Art. 34 recites, "For nomenclatural purposes, a species or any taxon below the rank of species is regarded as the sum of its lower taxa, if any *(sic)*. The description of a subordinated taxon which does not include the nomenclatural type of the higher taxon automatically creates a second subordinated taxon of the same rank which has as its nomenclatural type the type of the higher taxon (see Art. 35). Example: The publication in 1843 of *Lycopodium inundatum* var. *bigelowii* Tuckerm. automatically creates another variety which is the type of *Lycopodium inundatum*".

This thick verbiage can be pierced only by making reference to the example. Its authors hazily have in mind something which is the publication of the typical trinomial, which publication they would like from now on to take place according to certain esoteric formulae, something answering Ewan's outlandish wish (see main text) to have a "Definite name of reference without involving nomenclature." I will factually observe, i) It is not only a species, or a taxon below it, that is to be regarded as the sum of its lower taxa. A genus is the sum of its subgenera; a family of its subfamilies, etc. Accordingly, the introductory statement in Art. 34 is a misleading platitude without relation to the purpose and body of said Article. ii) It is false *as a matter of plain fact* that the publication of *Lycopodium inundatum* var. *bigelowii* "Automatically creates a second subordinated taxon of the same rank ..... another variety which is the type of *Lycopodium inundatum*". The publication of *L. inundatum* var. *bigelowii* segregates a variety out of the general concept expressed by the binomial *L. inundatum*. With var. *bigelowii* out, this binomial still includes — together with its typical part — whatever other unpublished variety stand beyond the limits of *bigelowii*. Patently, the publication of *one* trinomial under a species does not "Automatically create" every other trinomial, with or without the typical one to boot. Each publication is an end by itself, a deed released to the botanical polity under the authority of somebody, in a certain manner, at a certain date, in the pages of a certain printed text. Nothing of this can be "Automatically created", for everything of this is bound to answer the doings of an author acting in agreement with apt rules. iii) The concept of "Automatic creation" stands in flat opposition to all normal principles of publication, principles upheld even in the Stockholm Code, 1950. These principles demand that a taxon — *any taxon* — be released in a manner making it possible to properly and clearly report, list, use, understand and synonymize it. Is it not the notion fantastic, that when publishing *L. inundatum* var. *bigelowii* in 1843, Tuckermann "Implicity created" *L. inundatum inundatum*? Where precisely was this "implicit creation" effected and when, and by whom? How are we going to list it? Moreover, if anybody ever "implicitly created" *L. inundatum inundatum* it was Linnaeus himself, the author of the binomial, not Tuckermann. In sum: *What* do the authors of Art. 34 truly intend? *Why* do they intend it? *How* do they harmonize it with the principles of nomenclature in the very Code of Stockholm? *Let them speak clearer language than the text of their Article, if they can, withholding meanwhile their mischief.*

Further still: Art. 35 recites, "If any infraspecific taxon which includes the nomenclatural type of the epithet of the next higher taxon is to be mentioned as a subdivisional name, that name must repeat the epithet of the higher taxon unaltered but, contrary to Art. 55, without citation of an author's name ..... Examples: The binary combination *Lobelia*

*spicata* Lam. var. *originalis* Mc Vaugh which includes the type of *Lobelia spicata* Lam. must be altered to *Lobelia spicata* var. *spicata*".

Under the Rules in vigor to 1950, Mc Vaugh's *Lobelia spicata* Lam. var. *originalis* (and all trinomials in *typicus, vulgaris,* etc.) had legitimate status, and gave assuredly rise to no confusion. All that could be desired were minor rulings, how to transfer on occasion trinomials of the kind to other binomials, which rulings could easily be provided. With Art. 35 now on our back, a stupenduous, needless synonymy is created on account of replacing every trinomial using *typicus, vulgaris, originalis,* etc., with a repetition of the specific epithet. Homeric indeed is the joke attending the creation of this stupenduous needless synonymy: The typical trinomial to fit Art. 35 must yield no indication of author, when it is perfectly clear that *Lobelia spicata* var. *spicata* is defined by a specimen, liable to fall into synonymy, necessarily entered in all sorts of taxonomic indices and monographs, in sum, properly to be viewed, and treated, as one of the varieties into which *L. spicata* naturally decomposes in the field, in herbarium, and in the literature alike. If I set my hand conscientiously to write a botanic monograph, I am duty-bound carefully to return all publications, and well accounted for with name of author, date place of original release. In the name of what nomenclatural principles; to serve what fancied standard of theory and practical use, must I, when coming to a typical trinomial, reverse every form of normal procedure, and indulge, for example, in this crass nonsense, "*Lobelia spicata* Lam. var. *spicata* is a trinomial for which, much to my regret (but see Art. 35, Stockholm Code), I cannot give the name of an author. This trinomial, indeed, is now a "Binary combination" (see Art. 35, Examples) which, absurd on its face is nevertheless the official truth. Of course, *L. spicata* var. *spicata* was "Automatically created" (see Art. 34) at the time when a first variety was segregated out of *L. spicata,* but I cannot learn from the Stockholm Code, 1950, who is the "automatic creator" of *L. spicata* var. *spicata.* I should think it is Lamarck, though the principle of "Automatic creation" is absolutely repugnant to every nomenclatural concept, which demands that taxa, whatever their rank and quality, be clearly defined in relation to an author, a date, and a place of publication. Thinking it is Lamarck I may be wrong, nevertheless, for I will have to look up who first published a variety under *L. spicata,* and the name of this chap may be that of the "automatic creator" of that trinomial (rather, "binary combination"). I pray to the Almighty that the author of the first publication under *L. spicata* has clearly identified its rank, whether variety, subspecies or form, because, failing this, I no longer know what to do. Anyhow, it should be useless to learn the name of the "automatic creator" of *L. spicata* var. *spicata,* considering that this name cannot be affixed to the trinomial — *be it only to identify it for purposes of orderly recording* — by explicit fiat of Art. 35 in headlong defiance of Art. 55, which latter truly embodies fundamentals of genuine nomenclature.

Is there any reason, gentlemen wilfully puttering with nomenclature, and hanging from the neck of your over-kind colleagues such gross nonsense, why you should not be told, at long last overdue: *You do not know what you are doing, nor will you ever reform, because the record is final that you have been busy at it some 50 years already. Will you, please, stay out of this field, which you do not at all understand. Your "Type Method" is a parody, shot through with a fundamental misconception of "type" to agree with the crass "philosophy" of Britton & Co. Your Articles 34 and 35 are monstrosities answering esoteric notions of "type" in headlong conflict with nomenclatural principles tested in over a century of theory and practice, nor will such Articles ever factually work. In the wake of your misguided efforts ride darkness, disorder, confusion, decadence. Stay out of this, and let common sense speak again precise language. Nobody needs you: Go, learn but do not teach. You cannot teach, anyhow.*

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# Comments on Prof. Baehni's Proposition n. 4

[Taxon 2(4): 90-92]

by

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The proposals which follow are designed to meet Prof. Baehni's criticisms of the wording of certain Articles and Recommendations in the International Code, as some of the new English wordings proposed by him are not altogether satisfactory. It is desirable that such purely textual emendations should be discussed fully before the Paris Congress, so as to leave more time for consideration of proposals affecting the actual substance of the Code.

### Art. 13

The second sentence, to read as follows:

"Thus subfamily *(subfamilia)* denotes a category between familiy and tribe, subtribe *(subtribus)* a category between tribe and genus, etc."

*Argument.* As Prof. Baehni has pointed out, the word "a" in Art. 13, before "family", "tribe" and "genus", is inappropriate. Its omission meets this objection. The wording proposed above is in accordance with that of the Examples: "Series and subseries are categories which may be intercalated between subsection and species."

### Art. 20

To read as follows, the words italicized being inserted:

"The nomenclatural type of an order of any taxon of a rank between order and family is the family whose name *or alternative name* (see Art. 28) is based on the same generic name, that of a family or of any taxon between family and genus is the genus on whose present or former name that of the taxon concerned is based, and that of a genus or of any taxon between genus and species is a species.

Note. It is felt that the type method cannot at present be applied profitably to the nomenclature of taxa above the rank of order."

*Argument.* The insertion of the words "or alternative name" is designed to bring Art. 20 into conformity with Art. 27 and 28.

### Art. 23

In English there seems to be no objection to the phrase "Valid publication is treated as beginning ..... at the following dates". On the other hand, the statement that "The nomenclature of Fossil Plants begins with the year 1820" is incorrect, as pointed out by Prof. Baehni. But his proposed new version, "Pour tous les groupes de plantes fossiles, la validité de la nomenclature commence avec l'année 1820," is also wrong. The term valid applies solely to publication, see Art. 10. The following new version is accordingly proposed (after "h"):

"For fossil plants of all groups, valid publication is treated as beginning with the year 1820."

### Art. 27

To read as follows:

"The name of an order is taken from that of its type family with the ending *-ales.* If a conserved name of the type family is not formed from that of a genus, or if it does not end in *-aceae*, the name of the order is taken from the alternative name of the type family (see Art. 28).

Suborders are designated in a similar manner, with the ending *-ineae."*

"Examples of names of orders: *Fucales* (from *Fucaceae); Polygonales* (from *Polygonaceae); Urticales* (from *Urticaceae); Apiales* (from the alternative name *Apiaceae),* not *Umbellales* (from the conserved name *Umbelliferae); Brassicales* (from the alternative name *Brassicaceae),* not *Cruciales* (from the conserved name *Cruciferae); Arecales* (from the alternative name *Arecaceae),* not *Palmales* (from the conserved name *Palmae).*

Suborders: *Bromeliineae, Malvineae.*

*Argument.* The effect of Art. 20 is that names of orders can be based only on such names of families as are based on generic names. This excludes descriptive names such as *Parietales* and *Centrospermae,* as well as those based on the family names retained as exceptions under Art. 28. The insertion of the second sentence brings Art. 27 into conformity with Art. 20. As it might be argued that the conserved family name *Palmae* was based on the pre-Linnaean generic name *Palma,* the phrase "or if it does not end in *-aceae"* has been included.

### Art. 33 and Art. 34

There seems to be no objection to inserting

the words "number and case" after "gender" in these two Articles, if this is considered desirable for the benefit of those unacquainted with the Rules of Latin grammar. If it is necessary to include "number", as suggested by Prof. Baehni, surely it is equally necessary to include "case", e.g., *"Cinna arundinacea* .... Gramen magnitudine *Avenae sativae"* L. Sp. Pl. ed. 1, p. 5 (1753).

**Art. 43**

Prof. Baehni's criticism of the wording *"Cotema* Britt. & Wils. .... is a nomenclatural synonym of *Spirotecoma"* seems to be due to misconception. Synonyms are of two kinds: *nomenclatural synonyms,* i.e. names which are based on the *same type,* and are therefore synonyms under the *rules of nomenclature,* and *taxonomic* synonyms, i.e., those which are based on *different types,* and are synonyms according to a particular *classification.* These two kinds of synonyms were termed by Briquet "synonymes de nomenclature" and "synonymes taxonomiques" respectively — see Report of Proceedings, Fifth International Botanical Congress, Cambridge, 1930, p. 601 (1931). If the expression "synonyme nominal" in the French version is a tautological monstrosity, why not replace it by "synonyme de nomenclature"?

**Art. 53**

If the sentence "In the absence of proof to the contrary, the date given in the work containing the name or epithet must be treated as correct" is omitted, as suggested by Prof. Baehni, it will be necessary to insert a cross-reference to Art. 40. It seems much better to have all the provisions concerning dates of valid publication *in one place.* Cross-references should be avoided as far as possible.

**Art. 58 and Rec. 58A**

The two cases dealt with in Art. 58 and Rec. 58A are not comparable, and should not be combined under one Article.

Art. 58 deals with the *authority* for a name: if it is desired, as a matter of courtesy or historic interest, to cite the name of the author of a nomen nudum, his name may be inserted, followed by "ex", before the name of the publishing author who supplied the description, and in that case it forms part of the "authority", e.g., *Havetia flexilis* Spruce ex Planch. & Triana, though it may be omitted if desired.

Rec. 58A on the other hand, deals with the form of *bibliographic citation,* a minor matter unsuitable for inclusion in an Article. Thus *Viburnum ternatum* Rehder is the name for that species and the authority. The rest of the example, namely, "in Sargent, Trees & Shrubs, 2 : 37. 1907" is merely a bibliographic citation.

**Rec. 60G**

Replace the word "valid" by "the correct one for the genus", and insert the word "its" before "being". The Recommendation will then read as follows:

"If a generic name antedated by one of its synonyms or by a homonym is the correct one for the genus on account of its being a *nomen conservandum,* the words "nom. conserv." should be added to the citation."

*Argument.* Prof. Baehni rightly points out that it is only *publication* which can be *valid.* Reference to Art. 10 shows that the *correct name* of a taxon is the one which must be adopted for it under the rules. A generic name may have been either legitimate or illegitimate at the time of its publication, but in either case it becomes the *correct name* if it is conserved under Art. 24.

**Art. 65, examples**

Prof. Baehni here seems to be confusing taxonomy with nomenclature, and genera with generic names. The words *Pinus* and *Tsuga* may mean *either* the genera so named, *or* the generic names themselves. The *taxonomic* change actually made by Carrière was the transference of the species *Abies heterophylla* Raf. (which he erroneously identified with *P. mertensiana* Bong.) to the genus *Tsuga.* The *nomenclatural* change made by him was the transference of the specific epithet *mertensiana* from the generic name *Pinus* to the generic name *Tsuga.* There seems to be no reason whatsoever for altering the wording of the example.

**Art. 69**

Prof. Baehni has suggested that the words "the first *valid* name or epithet" should be replaced by "the first *legitimate* name or epithet." But a name or epithet may be legitimate, and yet not validly published. Hence it seems desirable that the phrase should read "the first *validly published legitimate* name or epithet."

It is true that, in the Code, unless otherwise indicated, the word "name" means a name which has been validly published, whether it is legitimate or illegitimate (see

Art. 10, Note), but as this provision might easily be overlooked by those consulting Art. 69, it seems better to make the wording of that Article perfectly explicit.

**Art. 73**

A name may be superfluous for one of two reasons, either on taxonomic grounds, e.g., *Cucubalus latifolius* Mill. and *C. angustifolius* Mill., or on nomenclatural grounds, e.g., *Chrysophyllum sericeum* Salisb. The expression "taxonomically superfluous" indicates that a name is superfluous because of the adoption of *a particular classification,* whereas the expression "nomenclaturally superfluous" indicates that it is superfluous under *the rules of nomenclature,* whatever classification is adopted. Prof. Baehni's criticism seems to lack justification.

**Art. 76**

Prof. Baehni rightly points out that, grammatically speaking, the word "its" in the first sentence should refer to "a name", whereas it is actually meant to refer to "a taxon". The first sentence may accordingly be amended to read as follows, the words "its characters" being replaced by "the characters of that taxon":

"A name of a taxon must be rejected if the characters of that taxon were derived from two ore more entirely discordant elements, unless it is possible to select one of these elements as a satisfactory type".

**Rec. 82C**

The first sentence should read: "When a new specific or *infraspecific* epithet", etc. The recommendation was presumably meant to apply to epithets of varieties, as well as to those of species and subspecies.

**Rec. 82F**

The first sentence should read: "New specific *or infraspecific* epithets", etc. The expression "new epithets" suggested by Prof. Baehni, would include "epithets" of subdivisions of genera, which were not intended to be covered by Rec. 82F.

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# OBITUARY*

Miriam Lucile Bomhard, range conservationist, U. S. Forest Service, died at her home in Glenshaw, Pa., on December 16, 1952, after a lingering illness. Miss Bomhard was born in Bellevue, Ky., on July 24, 1898, the daughter of the Rev. W. A. Bomhard and Emma Koch Bomhard. The family moved to Pittsburgh in 1907 from Owensboro, Ky. Miss Bomhard graduated from Sharpsburg, Pa., High School in 1917 as valedictorian, entering the University of Pittsburgh in the fall of that year on an honor scholarship. She graduated therefrom in February 1921 with the B.S. degree *cum laude* and in June of the same year received the M.A. degree. One summer was spent at the Cold Springs Harbor, Long Island, Laboratory of Biological Research, and from 1921 to 1925, including summer sessions, she served first as graduate assistant and then as instructor in the botany department of the University of Pittsburgh, meanwhile pursuing postgraduate studies in both botany and zoology. From 1925 to 1926 she was engaged in research work in the Carnegie Museum Herbarium. In 1926 she received the Ph.D. degree from the University of Pittsburgh, the first woman to receive that degree from that university. Her doctorate thesis, which covered illustrations and keys on the identification of seeds of Allegheny County, Pa., plants, unfortunately was never published except in abstract form. In 1926 she was appointed instructor in biology in Newcomb College, the women's department of Tulane University, New Orleans, the first courses in zoology and botany at that college, and in 1927 was promoted to assistant professor, a position she occupied until 1932, when she resigned to go to Malaya and for a trip around the world. Returning to the United States in 1933, at a time when new teaching positions were scarce, she accepted an appointment as junior pathologist in the Bureau of Plant Industry, Washington, D. C., and, from 1934 to her death, was in research work in the Forest Service in what is now the Division of Dendrology and Range Forage Investigations.

Dr Bomhard was a member of the American Association for the Advancement of Science, the American Society of Plant Taxonomists, the Botanical Society of America, the Botanical Society of Washington (corresponding secretary, 1940), the Ecological Society of America, the International Association for Plant Taxonomy, and the Washington Academy of Sciences. She was a cofounder of the New Orleans Society of Plant

Sciences, of which she was secretary in 1930 and president in 1931; also an associate member of the Society of American Foresters.

She was an authority on the taxonomy and ecology of United States range plants, especially of the Southeast, and on the taxonomy, distribution, and economic values of palms. Her interest in and knowledge of palms brought her inquiries from all over the world and from defense agencies and others in Washington concerned with important palm products. She served as taxonomist for the Vegetable Oil Mission of FAO in Venezuela in 1948. She was a contributor to the Forest Service *Range plant handbook* (1937) and, in connection with that work, developed a leafvenation method of distinguishing the highly poisonous waterhemlock genus *(Cicuta)* from the superficially similar innocuous angelicas. Among her more important publications were *The wax palms* (1930), a series of articles on palms in the Louisiana Conservation Review (1937-1939), the palm list in *Standardized plant names* (1942), and *Palm trees in the United States* (1950); she was also coauthor of *Field book of forage plants on longleaf pine—bluestem ranges* of Louisana (1952). During her earlier days she was a vocalist and sang on the radio. Because of her friendly, intelligent, and vivacious personality, wide travel and correspondence, and her services as guide to the A.A.A.S. meeting in New Orleans in 1931 for world scientists, Miss Bomhard had an enormous acquaintance, and her loss will be widely felt.

W. A. Dayton

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## An additional note on *Citrullus* Forskål, nom. conserv. propos.

by

Hiroshi Hara (Tokyo)

Fosberg recently published his opinion (Taxon 2(4): 99-101) regarding my proposal to conserve the generic name *Citrullus* for the watermelon. I was glad to know that he came to the same conclusion, viz. that *Citrullus* is to be conserved against *Anguria* and *Colocynthis*. Fosberg, however, considered that *Citrullus* should be adopted from Schrader (1836), and not from Forskål (1775), as I had proposed. The difference between our opinions seems partly to be caused by the fact that Fosberg did not fully understand my discussion in Bot. Mag. Tokyo 61: 1 & 4 (1948), which was partly written in Japanese. So I would like to explain here my intention once more.

Although I was aware of the rather inadequate form of Forskål's publication, this did not deter me. My main reason for proposing to base *Citrullus* on the work of Forskål (1775), was to avoid the necessity of introducing a new binary combination for the watermelon. If *Citrullus* is conserved from Schrader (1836), a new combination for the watermelon, via *Citrullus Anguria* (Dechesne), is inevitable under the present Code, for if *Citrullus Battich* Forskål is rejected, the earliest available binary name is *Cucurbita Anguria* Dechesne in Lamarck, Encycl. Méth. 2: 150 & 158 (1786). However, the specific epithet *Anguria* is not desirable for the Watermelon, because *Anguria* has been widely used for another genus of *Cucurbitaceae;* it would therefore become a source of confusion.

The next question is the validity of Forskål's publication. In his Flora Aegyptico-Arabica (1775) he clearly employed the Linnean system of binary nomenclature, and the adoption of a vernacular name as a specific epithet is not uncommon and is permissible under the Code. Moreover, *Citrullus Battich* was published with description, and in the same form as the other new species in his book; therefore it is quite natural to regard the binomial *Citrullus Battich* as validly published.

Though Forskål described three species under *Citrullus*, the first was the only for which he introduced a binominal, viz. *C. Battich*. The fact that the two other species probably belong to other genera, is of no importance for the solution of our nomenclatorial problem. In my opinion Forskål's *Citrullus* with only one validly published binomial, viz. *C. Battich*, may be regarded as a monotypic genus. This is a very rare case, and this interpretation does not affect other cases. The basic principle of Art. 24

seems to be the wish to avoid disadvantageous changes by the strict application of the rules in genera, which, like *Citrullus* Forskål, have long been in general use.

So I again propose to conserve *Citrullus* Forskål (1775), typified by *C. Battich,* for the watermelon. I hope that the question whether *Citrullus* should be conserved and dated either from Forskål (1775), which means that it is based on *C. Battich,* or from Schrader (1836), in which case a new combination, *C. Anguria,* is necessary, will be discussed at the coming congress.

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## Reinstatement of *Eranthemum pulchellum* Andr.

by

F. R. Fosberg and M. H. Sachet

The common blue eranthum, native of India and commonly planted as an ornamental in tropical gardens and in greenhouses, has received considerable mistreatment, nomenclaturally. In recent years it has usually been known as *Eranthemum nervosum* (Vahl) R. Br. ex R. & S., but almost as commonly as *Daedalacanthus nervosus* (Vahl) T. Anders., neither of which names appear to be correct. The earliest name available for this species clearly seems to be *Eranthemum pulchellum* Andrews, of which the exact date is not known but is earlier than 1800 and probably at least as early as 1798. The following annotated synonymy is intended to clarify the nomenclatural history of this species.

1. *Eranthemum pulchellum* Andrews Botanists' Repository 2: *pl. 88.* [1798?].

This name was effectively published and accompanied by a good description and plate and seems unquestionably to be the common blueflowered plant with imbricate variegated bracts. The tips of several of the bracts are slightly divergent, rather than erect as is usual for the species, but an occasional variant shows this character (i.e. *Purpus 17008,* and *Colani? 2902,* U.S. Nat. Herb.). It is by no means sufficiently marked to justify its reference in the Index Kewensis to the rather different, but related *E. purpurascens* Wight ex Nees. The latter has green bracts which are strongly divergent at apex, less imbricate, and longer flowers with narrow limb purple rather than blue in color. All volumes of Andrews Botanists' Repository bear the date 1797 on the outside, as does vol. 1 on the title page. The other title pages are undated. Vol. 2 has generally been cited as 1800. The parts were issued separately and Nakai, Jour. Jap. Bot. 17: 422-424. 1941, lists supposed dates of publication for all of the plates, giving April 1800 for plate 88. The text of his article is in Japanese and is not available in translation, but two things suggest that he may be wrong, both of which are pertinent to our point here. In 1798 Roxburgh, referred to *E. pulchellum* Andr. in synonymy of *Justicia pulchella,* and in 1800 Ventenat placed it in synonymy of his *Ruellia varians (vide infra).*

2. *Justicia pulchella* Roxb. Pl. Corom. 2: 41. *t. 177.* 1798.

This is obviously based on *E. pulchellum* Andr., as the latter is cited as a synonym with a complete reference except for the date. This may be taken as proof that Andrews' name appeared earlier than Roxburgh's second volume. The fact that Roxburgh's plant is definitely not the same as that of Andrews, but probably *E. purpurascens* is immaterial in light of Article 65 of the International Code of Botanical Nomenclature.

3. *Ruellia varians* Vent. Descr. Pl. Nouv. Hort. Cels. *t. 46.* [1800].

This likewise has *E. pulchellum* Andrews cited in synonymy, making *Ruellia varians* superfluous when published and a synonym of *E. pulchellum,* although the plant described and figured may not belong here. Willdenow later reduced *Ruellia varians* to *E. pulchellum,* Hort. Berol. Suppl. 2. 1813.

4. *Justicia nervosa* Vahl, Enum. Pl. 1: 164. 1804.

Based on a Röttler specimen from India, but clearly belonging to *E. pulchellum.* Aiton, Hort. Kew. ed. 2. 1: 42. 1810, places *Ruellia varians* Vent., *E. pulchellum* Andr. and *Justicia pulchella* Roxb. in synonymy here.

5. *Eranthemum pulchellum* w. [Willd.?] in Allg. Deutsch. Gartenmag. 7: 176, *t. 17,* 1810.

The plant illustrated is clearly *E. pulchellum* and if the "w" is assumed to be Willdenow, the name clearly is derived from that

of Andrews. It has, however, generally been regarded as a separate publication and was referred by Nees ab Esenbeck, in DC. Prodr. 9: 456. 1847, to *Eranthemum bicolor* Schrank, where it does not belong, leading Merrill, much later, into error *(vide infra)*. Nees, op. cit. 446, follows Aiton's lead and refers *E. pulchellum* to *E. nervosum* R. Br. It is probably the weight of Nees' authority on both of these points that perpetuated and increased the confusion surrounding these names.

6. *Eranthemum nervosum* R. Br. ex R. & S. Syst. 1: 174. 1817.

This was based on *Justicia nervosa* Vahl and, taken up by Nees, became for many years the generally used name for the plant.

7. *Daedalacanthus nervosus* T. Anders., Jour. Linn. Soc. 9: 487. 1867.

Based on *J. nervosa* Vahl. Anderson, realizing that, as currently applied, *Eranthemum L.* included two genera, supplied *Daedalacanthus* for one of them. Unfortunately he applied it to the one that included *E. capense* L., the original or type species of the genus. Radlkofer, Sitzungsber. math.-phys. Kl. Akad. Wiss. München 13: 285. 1883, recognized this error and restored *Eranthemum* in place of *Daedalacanthus* and gave the other genus, which Anderson had called *Eranthemum,* the name *Pseuderanthemum.*

8. *Pseuderanthemum pulchellum* (Hort.) Merr., Phil. Jour. Sci. Bot. 7: 248. 1912.

This was based by Merrill on the Gartenmagasin publication of *Eranthemum pulchellum,* apparently as a result of Nees' error in referring the latter to *Eranthemum bicolor* Schrank, which is a *Pseuderanthemum.* Merrill proposed *P. pulchellum* as an earlier name for *Pseuderanthemum bicolor* (Schrank) Radlk., but the combination actually belongs in the synonymy of *E. pulchellum* Andr. whether or not one considers that it is ultimately based on the latter name, as the plant described in the Gartenmagasin really belongs here, too.

It may be hoped, in conclusion, that no earlier epithet will be found for this attractive species, though it would not be surprising if one existed. It is a rather conspicuous plant to have remained unnamed until 1798.

Pacific Vegetation Project

## Proposals for additions to the Code of Nomenclature

**Proposal no. 27 submitted to the Paris Congres**

by

Rodolfo E. G. Pichi-Sermolli (Firenze)

The International Code of Botanical Nomenclature provides (Appendix I) a guide to the determination or selection of nomenclatural types of previously published taxa. This guide is a great improvement on the application of the type method (Section 2. Article 18-21), but, nevertheless, is not a sufficient means to fix unequivocally the nomenclatural type of some taxa, genera above all, the typification of which is particularly questionable and can be established only by means of the resolution of an International Botanical Congress.

It is a fact that the application of the old generic names depends chiefly on the selection of their type species. In several cases this selection is easy, and often, even if some authors disagree regarding some typification, the Guide to the determination of types leads us to an unequivocal selection. Sometimes, however, the selection of a lectotype of a genus is very difficult and the arguments adduced by the authors who have contrasting opinions on the subject appear equally good to anyone who is prepared to judge objectively. It follows that in some cases the same generic name is adopted by authors for different genera while, on the contrary, in other cases an uncertain typification results in the same genus having two different names, since not all authors accept the generic name of uncertain typification and some of them adopt for the genus a later name. At all events even if an uncertain typification does not affect the nomenclatural state of a genus, a selection of its lectotype species is necessary for the nomenclature of its subdivisional taxa.

In these cases the lectotype species of these genera of uncertain or questionable typification can only be established conventionally by means of the decision of an International Botanical Congress. At present the Code does not allow us to do so, as we cannot include a genus in the list of Genera Conservanda (Appendix V.B.) simply in order to fix its

type species. For these reasons I propose to add as an appendix to the International Code of Botanical Nomenclature a list of the lectotype species which are selected conventionally and must be retained.

In order to avoid the creation of new Committees I think that it would be advisable at least for the time being to refer the proposals for typification to the same Special Committees of Nomenclature for the various taxonomical groups as are appointed for the study of the proposals of the nomina conservanda.

I realize that this list will require a lot of work, but we must consider that it would be devoted chiefly to the genera the typification of which is uncertain. On the other hand we must remember that the opportuneness of this Appendix with the "Species Lectotypicae Generum" is demonstrated by the list of the lectotype species of the Linnean genera prepared by A. S. Hitchcock & M. L. Green which was published as a supplement to the last edition of the "International Rules of Botanical Nomenclature".

To sum up what I have noted above, I propose the creation of a new Appendix to the International Code of Botanical Nomenclature under the name of "Species Lectotypicae Generum", and consequently the addition to the same Code of the following new article:

**Art. 21 bis**

In order to fix the nomenclatural type of some genera, the typification of which is uncertain or disputed, this Code provides a list of the lectotype species of these genera, which are established by convention and must be retained (Appendix VII. Species Lectotypicae Generum). This list will remain permanently open for additions. Any proposal for the typification of a genus must be accompanied by a detailed study, with full bibliographical data, in which historical, taxonomical, and nomenclatural grounds for the typification of the genus are carefully and exhaustively discussed.

The proposals must be submitted to the General Committee for Botanical Nomenclature, which will refer them for examination to the Special Committee for the taxonomical group concerned. This Committee will submit its comment and voting on the proposals to the Advisory Board and the General Committee of Nomenclature which in its turn will take into consideration the proposals and will refer all the data on them to the International Botanical Congress for the final decision.

I hope that taxonomists realize the necessity of this list of the lectotype species of genera, which represents another important means for reaching stability and clarity in the nomenclature of genera and species.

* * *

The tendency of some authors to select doubtful or misinterpreted species as lectotypes of genera induces me to make two other proposals.

Sometimes authors on the basis of the strict application of Appendix I "Determination of types" of the Code of Nomenclature are led to select as the lectotype species of a genus a doubtful species. Since the principal aim of the type method is to fix the correct and unequivocal identity of a taxon, and on the other hand this depends chiefly on the sure identity of its type, I think that, whenever the lectotype of a taxon must be selected, the first qualification of this lectotype must be its certain identity, otherwise the typification becomes a source of confusion rather than of clarification.

This principle must be above all borne in mind in the selection of the type for a genus as in this case changes of names and the creation of new combinations could be involved owing to an unsuitable selection. For these reasons I think that it is senseless to select as the lectotype of a genus a doubtful species, even if according to the guide for the determination of types this selection ought to be the most suitable. In fact in so doing the identity of the genus concerned will result in its turn doubtful and we might be liable to misapply the generic name concerned and eventually be compelled to changes of names. Even if the doubtful species is to be referred unquestionably to the genus concerned, such a typification is not advisable. In fact, apart from the question of principle, I think that it is unsuitable that the type cannot be mentioned among the species of the genus.

Sometimes a genus consists of species previously described under another genus. When the author failed to designate the type of his new genus, a lectotype must be selected for it. Authors on the basis of a previous selection sometimes accept as lectotype of a genus a species which was incorrectly interpreted and erroneously named by the author of the genus itself. Clearly this is incorrect. In fact according to Article 65

"When on transference to another genus, the specific epithet has been applied erroneously in its new position to a different plant, the new combination must be retained for the plant on which the epithet was originally based, and must be attributed to the author who first published it." Consequently if we select as the lectotype of a genus a species which was misinterpreted and erroneously named by the author of that genus we find ourselves in the strange situation of selecting as lectotype of a genus a species which was not studied by the author of the genus itself; which is not compatible with the rule 4.b of the Appendix I. If, on the contrary, we select as the lectotype of that genus the species which was actually described by the author of the genus the lectotype species does not correspond to that which was selected as the type of the genus by previous authors, and on the other hand this lectotype species has a different author from the one who described the genus. In both cases such a selection is not advisable and I think that it is better for the sake of clarity to select another lectotype of that genus, if another species is equally qualified to be its type.

The guide for the determination of types (Appendix I of the Code of Nomenclature) does not take into account these two cases of doubtful and misinterpreted species and therefore I think it advisable to add to it the following two new rules:

**Appendix I**

**Determination of types**

8. A species of uncertain identity cannot be selected as the lectotype of a genus or any infrageneric taxon.

9. A species which was incorrectly interpreted and erroneously named by the author of a genus cannot be selected as lectotype of that genus.

7th June, 1953.

# Propositions pour le Congrès de Paris

**Proposition n. 28**

Proposition pour les modifications suivantes aux articles 28 et 29:

**Article 28**

Transformer ainsi le premier paragraphe: Un nom de famille est un adjectif au pluriel, utilisé comme substantif, formé par adjonction du suffixe *-aceae* au radical du nom du genre-type ou d'un synonyme au génitif.

**Article 29**

Transformer ainsi le premier paragraphe: Un nom de sous-famille est un adjectif pluriel, utilisé comme substantif, formé par adjonction du suffixe *-oideae* au radical du nom du genre-type ou d'un synonyme au génitif; les noms de tribus.... etc. (comme précédemment).

Commentaires: La rédaction actuelle de ces articles est d'une imprécision flagrante. Une proposition a déjà été publiée pour y remédier (Proposition no. 6, par Antonio Ponce de León y Aymé et Maria Teresa Alvarez; Taxon 2 (4): 96-98. 1953). Mais le remède est pire que le mal, car les auteurs supposent que le suffixe *-aceae* doit être accolé au radical du nom du genre-type au nominatif. D'où une complication extrême: 5 cas d'exceptions sont prévus (noms en *-r*, noms en *-as*, *-is* ou *-ans*, noms en *-x*, noms en *-ago*, noms à radical en *-i*), malgré lesquels les auteurs sont conduits à former des noms grammaticalement incorrects, Hydrocharidaceae et Melastomaceae (*loc. cit.* 98), et leurs propositions conduiraient à en former bien d'autres, tels que Saccharomycaceae (pour Saccharomycetaceae), Fissidaceae (pour Fissidentaceae), Phytelephadaceae (pour Phytelephantaceae) ou Vitidaceae (pour Vitaceae).

En fait, le radical d'un nom latin de la 3e déclinaison peut être tronqué ou altéré au nominatif, alors qu'il ne l'est jamais au génitif (exemple: *vis, viris,* rad. *vir-; flumen, fluminis,* rad. *flumin-; dux, ducis,* rad. *duc-*). C'est sur le radical du génitif que doivent être formés les noms de familles, comme du reste les noms de sous-familles, tribus et sous-tribus. Aucune exception n'est alors à prévoir. Rappelons que le génitif latin des noms grecs latinisés est calqué sur le génitif grec, la terminaison *-ος* étant simplement remplacée par la terminaison *-is*.

*Exemples tirés de la proposition no. 6*

*Papaver, Papaveris,* rad. *Papaver-: Papaver*aceae.

*Cycas, Cycadis,* rad. *Cycad-: Cycad*aceae.

*Iris, Iridis,* rad. *Irid-: Irid*aceae.

*Juglans, Juglandis,* rad. *Jugland-: Juglanda*ceae.

*Salix, Salicis,* rad. *Salic-: Salic*aceae.

*Borago, Boraginis,* rad. *Boragin-: Borragin*aceae.

*Nyctago* (et non *Nyctaginia*), *Nyctaginis,* rad. *Nyctagin-: Nyctagin*aceae.

*Exemples contredisant la proposition no. 6*

a. Noms grecs latinisés en *-as*. La plupart ont un génitif en *-adis* (*-αδος*), comme *Dryas, Dryadis* (*δρυας, δρυαδος*). Cependant: *Phytelephas, Phytelephantis* (*ἐλεφας, ἐλεφαντος*), rad. *Phytelephant-: Phytelephant*aceae. Noms formés arbitrairement par adjonction d'un suffixe *-as* qui n'existe pas en grec, pouvant être considérés comme indéclinables: *Helonias*, rad. *Heloni-: Helon*ieae; de même *Spondi*eae.

b. Noms grecs latinisés en *-es*. *Saccharomyces, Saccharomycetis* (*μυκης, μυκητος*), rad. *Saccharomycet-: Saccharomycet*aceae.

c. Noms grecs latinisés en *-is*. La plupart ont un génitif en *-idis* (*-ιδος*), comme *Iris, Iridis* (*ἰρις, ἰριδος*). Quelques-uns l'ont cependant en *-itis: Hydrocharis, Hydrocharitis* (*χαρις, χαριτος*), rad. *Hydrocharit-: Hydrocharit*aceae (et non Hydrocharidaceae); de même *Anacharit*eae, *Eucharit*inae (et non Eucharidinae).

d. Noms grecs latinisés en *-os*. La plupart répondent à la proposition no. 6; exemple: *Cissampelos, Cissampeli* (*ἀμπελος, ἀμπελου*), rad. *Cissampel-: Cissampel*inae. Cependant: *Anthoceros, Anthocerotis* (*κερας, κερατος*), rad. *Anthocerot-: Anthocerot*aceae.

e. Noms grecs latinisés en *-us*. *Trichopus. Trichopodis* (*πους, ποδος*), rad. *Trichopod-: Trichopod*aceae; de même *Trachypod*eae. Cependant: *Rhus, Rhois* (*ῥους, ῥοος*), rad. *Rho-: Rho*ëae (et non Rhoideae).

f. Noms grecs latinisés en *-ys*. *Ophrys, Ophryos* (*ὀφρυς, ὀφρυος*), rad. *Ophry-: Ophry*eae (et non Ophrydeae); de même *Hydrostachy*aceae, *Stachy*oideae. Cependant: *Aspidopterys, Aspidopterygis* (*πτερυξ, πτερυγος*), rad. *Aspidopteryg-: Aspidopteryg*eae. Nom „barbare" en *-ys* assimilé aux noms grecs en *-is: Mays, Maydis*, rad. *Mayd-: Mayd*eae.

g. Noms grecs latinisés en *-en. Cyclamen, Cyclaminis* (*κυκλαμινον, κυκλαμινου*), rad. *Cyclamin-: Cyclamin*eae.

h. Noms grecs latinisés en *-in. Triglochin, Triglochinis*, rad. *Triglochin-: Triglochin*eae.

i. Noms grecs latinisés en *-on*. Beaucoup répondent à la proposition no. 6; exemple: *Myzodendron, Myzodendri* (*δενδρον, δενδρου*) rad. *Myzodendr-: Myzodendr*aceae. Cependant: *Andropogon, Andropogonis* (*πωγων, πωγωνος*), rad. *Andropogon-: Andropogon*eae; de même *Potamogeton*aceae, *Aponogeton*aceae, *Gyrostemon*aceae, *Podostemon*aceae, *Pterostemon*oideae, *Croton*oideae, *Stylochiton*eae, *Glechon*eae, *Pogostemon*eae. Autre type: *Leucodon, Leucodontis* (*ὀδους, ὀδοντος*), rad. *Leucodont-: Leucodont*aceae; de même *Lepryrodont*aceae, *Rhegmatodont*aceae, *Siphonodont*aceae, *Stereodont*eae, *Trematodont*eae, *Diplusodont*inae.

j. Noms grecs latinisés en *-ma* (neutres). *Melastoma, Melastomatis* (*στομα, στοματος*), rad. *Melastomat-: Melastomat*aceae (et non Melastomaceae); de même *Alismat*aceae (et non Alismaceae), *Crocossosomat*aceae, *Geissolomat*aceae, *Pentaphragmat*aceae, *Dictyolomat*oideae, *Aglaonemat*eae (et non Aglaonemeae), *Staurostigmat*eae, *Polyosmat*eae, *Picrasmat*eae, *Arisaemat*inae, *Aspidospermat*inae, *Metastelmat*inae (et non Metastelmatineae: Code Int. Nom. Bot., trad. fr. 170. 1952), *Glossonemat*inae.

k. Noms grecs latinisés en *-ops*, formés arbitrairement par contraction du grec *ὀψις, ὀψεως*, et pouvant être déclinés sur le modèle du latin *ops, opis*: *Balanops, Balanopis*, rad. *Balanop-: Balanop*aceae (et non Balanopsaceae, ni Balanopsidaceae); de même *Dryobalanop*eae (et non Dryobalanopseae), *Mimusop*eae, *Echinop*inae (et non Echinopsinae).

l. Noms purement latins en *-is. Fontinalis, Fontinalis*, rad. *Fontinal-: Fontinal*aceae; de même *Cannab*aceae (et non Cannabinaceae), *Vit*aceae, *Mirabil*eae, *Sinap*eae, *Turrit*inae, *Mercurial*inae.

m. Noms purement latins en *-ens. Fissidens, Fissidentis*, rad. *Fissident-: Fissident*aceae; de même *Spiriden*taceae.

n. Noms purement latins en *-ex. Rumex, Rumicis*, rad. *Rumic-: Rumic*oideae; de même *Caric*oideae, *Vitic*oideae, *Atriplic*eae.

o. Noms purement latins en *-o. Restio, Restionis*, rad. *Restion-: Restion*aceae; de même *Senecion*eae. Noms „barbares" assimilés: *Nelumbon*oideae, *Durion*eae.

Proposé par: M. Pichon (Paris).

**Proposition n. 29**

Proposition pour la modification suivante de l'article 34:

**Article 34**

Transformer ainsi la fin du second alinea: „.... et dont le type nominal est alors, sauf inconvénient grave ou impossibilité, celui du taxon supérieur (cf. Art. 35)".

Commentaires: Il y a simplement addition des mots „sauf inconvénient grave ou impossibilité". La règle actuelle n'a pas prévu les cas tels que les suivants:

1° Supposons une espèce formée de deux variétés, l'une à feuilles larges, l'autre à feuilles étroites. Par définition, les variétés ne sont pas aussi nettement tranchées que les espèces. A côté des nombreux échantillons à feuilles larges et des nombreux échantillons à feuilles étroites peuvent exister quelques termes de passage. Si le type de l'espèce est l'un de ces échantillons ambigus, il y aurait *inconvénient grave* à le désigner comme type d'une des variétés.

2° Supposons une espèce de deux variétés qui ne se distinguent que par la couleur du fruit. Si l'échantillon type de l'espèce est en fleurs, il y a *impossibilité* de le désigner comme type d'une des variétés (sinon à pile ou face, procédé bien peu scientifique).

Dans des cas semblables, nous proposons que les deux variétés aient chacune un type différent du type de l'espèce et une épithète différente de l'épithète spécifique.

Proposé par: M. Pichon (Paris).

**Proposal no. 30**

Proposal to conserve the name **Bernardia** Müll. Arg.

The American genus *Bernardia* was described by Browne in *Nat. Hist. Jam.* 361, in 1756. However, this publication is to be eliminated from botanical literature since it does not follow the binomial nomenclature.

Adanson was the next author to describe the genus with the same name in *Fam. Plantes* 2: 356. 1763. Although the genus has been attributed to him by some authors, his description is so brief and so vague that is not possible to know which plant he refers to.

Müller Arg. described a number of species in this genus in *Linnaea* 34. 1865-66, but he did not give any generical description. It is only in 1866 that he describes the genus very accurately in DC. *Prodromus* 15 (2): 915. He assigns 27 species to it described by himself and amongst the synonyms he includes in the first place *"Bernardia Houst. ap. P. Browne Hist. Jam. plant. p. 361. pr. p."*

The genotype *Adelia Bernardia* L. is included in the sense of *Bernardia* Müll. Arg.

In taking up *Bernardia* Müll. Arg. there are several earlier synonyms, namely: *Bivonia* Spr., *Tyria, Phaedra, Polyboea* and *Traganthus* Kl. *Passaea* and *Alevia* Baill.

This proposal is based upon the facts:

1) The name *Bernardia* Houst. ex Browne is not to be considered valid since the publication does not follow the binomial nomenclature.
2) Adanson's description does not give either the idea of the genus or record of any genotype. Moreover, it has been proposed that Adanson's work "be rejected to prevent the invalidation of well-established names" (Cfr. Willmott, A. J. *Kew Bull.* 1935. 91-92).
3) The description of Müller Arg. is complete and it fulfils the requirements of the *Code*. Twenty-seven species are described under it.
4) Before 1866 only 3 species had been described (by Grisebach, 1861 and 1864) under *Bernardia*.
5) Since 1866, 34 species have been added to this genus.
6) On the contrary, no species has been described under the generic synonyms. The name has been adopted since 1866 by Müll. Arg. in Martius, *Fl. Brasil.*, Bentham & Hooker, *Gen. Plant.*, Pax and Pax & Hoffmann in the two editions of *Pflanzenfamilien* and in the *Pflanzenreich*, Lourteig and O'Donell, *Lilloa* 8, 1942 et *Gen. Sp. Pl. Arg.* 1, 1943 and Lourteig, *Arkiv f. Botanik*, 1952.

Proposed by: A. Lourteig, Dept. of Botany, Smithsonian Institution, Washington D.C.).

**Proposal no. 31**

Proposal to conserve the genus **Kniphofia** (Liliaceae);

1024. *Kniphofia* Moench, Meth. 631 (1794), non *Kniphofia* Scop., Introd. 327 (1777), *versus Tritoma* Ker, in Bot. Mag. t. 744 (1805).

*Kniphofia* Moench, *Liliaceae.*

More or less universally adopted by botanists with the exception of some American authors & horticulturists. Used by A. Berger in the latest revision of the genus in Engl. Pflanzenreich 4, 38, 3 (2): 31. 1908). Over 100 published names excluding a number of hybrids & garden forms.

Type species: *K. uvaria* (L.) Hook.

*Kniphofia* Scop. *Combretaceae.*

Only occurrence of the name is in the original publication. The native name of *Adamaram* cited after the description is the same as that listed for *Terminalia catappa* in Burman's Index Hort. Malab. 3 (1769). The name *Kniphofia* Scop. has never been used in combination with any specific epithet.

Type not designated.

*Tritoma* Ker, *Liliaceae.*

Genus described by Ker who was apparently unaware of *Kniphofia* Moench.

Three species of *Tritoma* mentioned in this work. Only 27 combinations published to date.

Type species: *T. media* Ker.

Discussion: The adoption of *Tritoma* would necessitate the creation of about 80 new combinations. The name *Kniphofia* Moench. is well known & established & is used not only in the most recent horticultural works, viz. Bailey Man. Cult. Plants 205 (1949) & R.H.S. Dict. Gard. 1106 (1951) but also in all the African literature.

Recommendation: to conserve *Kniphofia* Moench. (1794) over *Kniphofia* Scop. (1777).

Proposed by: E. A. Bruce (Kew).

**Proposal no. 32**

Proposal to conserve the genus **Pherosphaera** sensu Hooker.

Our present usage of the names of Tasmanian conifers follows Hooker's "Flora Tasmaniae" (1860). When the first collections of conifers were made in Tasmania there was considerable confusion over their identity. The genus *Microcachrys* was created by J. D. Hooker in 1845 for the species which W. Hooker called *"Athrotaxis" tetragona*, based on male plants only, since what he thought were its female cones were very different from those of *Athrotaxis*. The name *Pherosphaera hookeriana* was given by Archer in 1850 to the true female cones of the species Hooker had described as *Microcachrys tetragona*. Archer took to be *Microcachrys* the species subsequently described by Hooker as *Diselma archeri*. What Hooker at first believed to be female *Microcachrys* is the plant we now know as *Pherosphaera hookeriana*, but this is not the original *Pherosphaera* of Archer. *Pherosphaera* Archer is thus strictly a synonym of *Microcachrys*, and Garden and Johnson (Contr. N.S.W. Herb. 1 (6): 315-6. 1950) have proposed the name *Microstrobos* for *Pherosphaera* sensu Hooker; *P. hookeriana* becomes *Microstrobos niphophilus*, and *P. fitzgeraldi*, *M. fitzgeraldi*. (cf. fig. 1).

Fig. 1. *The situation around Phaerosphaera sensu Hooker*

| Species | A | B | C |
|---|---|---|---|
| W. Hooker 1843 | ♂ "Athrotaxis" tetragona<br>♀ — | — | — |
| J. D. Hooker 1845 | ♂ Microcachrys tetragona<br>♀ — | —<br>Microcachrys tetragona | — |
| Archer 1850 | Pherosphaera hookeriana | — | Microcachrys tetragona |
| J. D. Hooker 1860 | Microcachrys tetragona | Pherosphaera hookeriana | Diselma archeri |
| Garden and Johnson 1950 | Microcachrys tetragona | Microstrobos niphophilus | Diselma archeri |

The mistake was Hooker's in the first place in not recognising the true female plants of *Microcachrys*, and in Flora Tasmaniae he corrected all the previous errors and applied the names in what appeared to be a most reasonable way. The plants now called *Pherosphaera* are well known for their great morphological interest, and it would be regrettable to change the generic name for that reason. I consider that *Pherosphaera* sensu Hooker should be a nomen conservandum.

Proposed by: Charles G. Elliott (Cambridge, G.B.).

### *Special Committee for Phanerogams and Pteridophyta*

The Secretary, Prof. R. PICHI-SERMOLLI announces that Mr S. T. BLAKE, Brisbane, Australia is elected as a Member of the Subcommittee for Phanerogams. Mr. BLAKE takes the place of Mr W. D. FRANCIS from Brisbane, who offered his resignation.

### EMIL ISSLER †

Am 1. Sept. 1952 verstarb zu Colmar (Ht. Rhin), Frankreich, einige Tage vor seinem 80. Geburtstag, nach kurzer Krankheit, EMIL ISSLER, anerkannter Spezialist der Floristik und Pflanzengeographie der Vogesen, Verfasser der vorzüglichen Monographie: „Vegetationskunde der Vogesen".

### PELTIGERA

Mr P.-O. LINDAHL, Institution of Systematic Botany, University of Uppsala, Sweden, requests specimens of the genus Peltigera for identification.

### NATIONAL HERBARIUM, PRETORIA, SOUTH AFRICA

An event of the utmost importance in the history of the National Herbarium was the addition to it of the Transvaal Museum Herbarium in June 1953. This came about through the Museum Commission of 1949—1950, which recommended a policy of centralisation of study collections to faciliate research work. The Museum Herbarium contains about 40,000 mounted specimens including the original set of Miss Leendertz (Mrs. Leendertz Potts) and extensive sets from the collections of R. Schlechter, F. A. Rogers and others. The National Herbarium is now estimated to contain about 370,000 specimens.

### SHORT ACCOUNT OF TRANSVAAL MUSEUM BOTANICAL COLLECTION

The Botanical Collection of the Transvaal Museum was started with duplicate material from the well-known collection of Dr Rudolf Schlechter.

The first botanist, in the person of Miss Reino Leendertz (who studied under the famous Prof. Hugo de Vries, Amsterdam University), was appointed in Sept., 1898. Miss Leendertz left the Museum in October 1900, but rejoined in Jan., 1904 and continued as a botanist in charge until her retirement in Jan., 1924.

To Miss Leendertz must go full credit for building up the Museum's collection, from its small beginning of Schlechter's duplicates, to one of the most important assemblages of plants (especially of the Transvaal) in South Africa, totalling at the time of her retirement in 1924 just over 25,000 mounted sheets of plants; indeed a monument to her enthusiasm and industry.

Miss Leendertz was a most assiduous collector and in the early days spared no effort, not only to collect specimens herself by going out in the field by bicycle, ox-wagon or any other means of transport available, but also interested others all over the country to send material to the Museum; among these latter special mention may be made to the Rev. Henri Junod of Shilowane and the Rev. R. A. Rogers of E. Tvl., whose sendings made a considerable contribution to the Collection.

In addition to her actual collecting and assembling of material, Miss Leendertz maintained extensive contact with most of the other well-known botanists of her time, including among many others, L. Bolus, N. E. Brown, Burtt Davy, J. R. Sim and J. Marloth.

Following on Miss Leendertz (actually then Mrs. Leendertz-Pott) Miss D. Kaplan was in charge for a few years and was followed by Miss A. A. Obermeyer and Miss M. Lanham; after the resignation of the latter in July 1943, no further appointments were made for reasons of economy. During the period following Miss Leendertz, the most significant increases to the collection were made by Miss A. A. Obermeyer who, in collaboration with Prof. Bremekamp (then Prof. of Botany, Pretoria Univ. College) also worked on and wrote up accounts of the plants of the Kalahari and Kruger National Park.

At handing over the collection to Division of Botany on 22nd June, 1953, the total number of sheets of named and mounted plants totalled close on 38,000 plus some thousands of mounted sheets of unidentified and duplicate specimens and a collection of dried seed pods, etc.

R. A. DYER

### DAS HERBARIUM IN HALLE (SAALE)

Botanische Anstalten der Martin-Luther-Universität, Halle-Wittenberg, Halle (Saale), Am Kirchtor 1.

Den Grundstock dieser Sammlung bildet das Herbarium der Universität Wittenberg aus dem 18. Jahrhundert, das in der Hauptsache von SCHKUHR stammte und dessen gesamtes Herbar enthält. Bei der Vereinigung der Herbarien Wittenberg und Halle wurde diese Sammlung 1814 nach Halle überführt und von ANTON SPRENGEL mit dem wenig umfangreichen Hallenser Material 1825 vereinigt und neu geordnet. Der Sprengel'sche Index ist erhalten und umfasst 4 400 Arten. In ihm befinden sich u.a. Exemplare von Swartz, Roth, Hedwig und Bestimmungsbemerkungen von Curt Sprengel. (Das Herbarium Sprengel selbst verblieb nicht in Halle, ein Teil davon gelangte an das Botan. Museum, Berlin-Dahlem). Später kamen die Sammlungen des Berliner Arztes Heim, besonders wertvoll durch seine Moose, mit Exemplaren von Masson, Banks, Dillenius u.a. dazu. Weiterhin wurden erworben 1 500 Pflanzen von Sello, weitere von Mundt, Riedel, Spitzel, Schimper, Lhotsky, Hampe und einiges Material von Schlechtendal. Diese Sammlung umfasste etwa 7 600 Arten wie ein 1829 von Thilo Irmisch angelegtes, noch vorhandenes Verzeichnis ausweist.

Später wurde mit diesen Sammlungen das Material von Schlechtendal vereinigt, das ausserordentlich wertvolle Exemplare enthält. Das gesamte Herbar umfasst damit etwa 80.000 Bogen, hat aber seit 1850 keinen nennenswerten Zuwachs mehr erhalten. An wichtigsten Sammlern wären noch zu nennen: Hohenacker, Humboldt, Chamisso, Ehrenberg, Schiede und Deppe, Martius, Sieber, Ecklon und Zeyher, E. Meyer, Kotschy, Scholzki, Moritz, Schomburgk, Salzmann, Makino, Pabst, Garcke, Fuckel, G. v. Hoffmannsegg.

Eine in neuerer Zeit erworbene, gesondert aufbewahrte Sammlung ist das Herbar Sporleder (Flora von Wernigerode).

W. ROTHMALER

## THE NATAL HERBARIUM, DURBAN, SOUTH AFRICA

When in 1882 the late Dr Medley Wood became curator of the Botanic Gardens, he found a small parcel of about 400 unmounted specimens collected chiefly by McKen and Keit, former curators, and Gerrard. These specimens, together with his own small private collection, formed the nucleus of the present Natal Herbarium.

Medley Wood went on extensive collecting trips all over Natal and Zululand and, over the years, collected thousands of plants, many of them new to science. Later Maurice Evans and Thode, who collected chiefly in the northern districts and Drakensberg, and Haygarth and Wylie, who collected in the coastal, midland and Zululand areas, also added new and valuable plants to the collection. All have had plants named in their honour.

Medley Wood sent parcels to Kew and exchanged specimens with many European and foreign herbaria, as well as with South African herbaria. In this way specimens collected by Drege, Ecklon and Zeyher, Burchell, MacOwan, Tyson, Schlechter, Bolus and Buchanan were received, and in the tropical African Section from, among others, Schimper and Schweinfurth.

When the present Herbarium buildings were opened in 1902 there were 28,879 specimens in the collection.

Medley Wood described many new species himself and collaborated with Evans in describing others. He published several botanical papers, but his two most important works were "Natal Plants", Vol. 1—6, and a "Handbook to the Flora of Natal".

Shortly after Union the Herbarium was taken over by the Government Dept. of Agriculture, Medley Wood retaining his position as Director till his death in 1915. From 1915 to 1919 Dr van der Bijl was in charge, but, as he was a Mycologist, practically no botanical work was done during that period.

There are at present 94,360 specimens in the herbarium mainly plants from Natal and Zululand. The collection is a valuable one as there are a large number of types and type-numbers, as well as specimens cited in "Flora Capensis" and "Flora of Tropical Africa".

H. M. L. FORBES

## QUEBEC AND NEWFOUNDLAND DUPLICATES AVAILABLE

Owing to the joining of the personal herbarium of the late Brother MARIE-VICTORIN with the one of the University of Montreal, a considerable series of Quebec plants will be available for exchange. Specialists are invited to write for their desiderata. Series of Newfoundland plants will also be available in a near future. Please address correspondence as follows: Dr ERNEST ROULEAU, Herbier Marie-Victorin, Université de Montréal, 4101 est rue Sherbrooke, Montréal 36, Canada.

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